

A statistician's view of weather modification technology (A Review)

(drought/national policy/randomized experiments/operational cloud seeding)

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The present paper is motivated by the enactment, in October 1976, of Public Law 94-490 which directs the U.S. Secretary of Commerce to formulate an appropriate national policy on weather modification. The current widespread drought contributes to the interest in the present state of this art. Here, then, the question of national policy becomes combined with the necessity of a more or less immediate decision on the use of public funds to pay for cloud seeding operations (as distinct from experiments) with the hope of increasing precipitation where it is most needed. The following presents a series of facts deemed relevant both to the immediate question of alleviating the drought and to the more general problem of long-range national policy. The specific questions considered and the present writer's answers are as follows.

Question i. Is the present cloud seeding technology reliably confirmed as a means of alleviating drought? *Answer:* No.

Question ii. Is there evidence that cloud seeding affects precipitation and, if so, what are the indicated effects? *Answer:* It appears established that cloud seeding does affect precipitation and does so over areas far in excess of the intended targets, occasionally up to distances on the order of hundreds of kilometers. In some cases the effects are large increases and in some others large decreases in precipitation. The several hypothetical mechanisms advanced to explain these effects and to predict them vary in their empirical support and convincingness. In particular, much of the existing literature, some of it stemming from official sources, is slanted and unreliable.

Question iii. What are the means of advancing the development of a reliable weather modification technology? *Answer:* Establishment of at least two philosophically different interdisciplinary research groups, including statisticians versed in experimental work, perhaps members of the National Academy of Sciences, with a special mission to reevaluate the data of as many already performed cloud seeding experiments as possible, and continuation of properly planned experimentation. The suggested research groups should have unlimited access to the same data and have facilities for personal meetings to exchange ideas. They should be funded from sources other than those engaged in funding cloud seeding.

Remark. The preliminary draft of this paper was sent to a number of knowledgeable persons with a request for comments. Some of the comments received caused certain changes in the original formulation. Some other comments indicated the desirability of response. Brief summaries of such comments, followed by my response, are interspersed in the paper. The beginnings and the ends of such insertions are marked by the symbol [CR]. The author is very grateful to all these scientists who read the original draft and summarized their reactions.

HISTORICAL BACKGROUND

The problem of weather modification is quite old and highly controversial. The *Smithsonian Report* of 1894 carried an ar-

ticle by M. W. Harrington (1) under the telling title "Weather making, ancient and modern." Apparently, the "modern" weather making of the time included some methods that were patented. However, broad public interest in the possibility of modifying weather, primarily by increasing rain or by preventing hail, developed much later. This occurred after the publication of the pioneer works of Langmuir (2), of Schaefer (3), and particularly of Vonnegut (4) indicating that the introduction of silver iodide (AgI) smoke into a supercooled cloud of water droplets will produce ice crystals. These results were publicized and a wave of AgI cloud seeding enthusiasm spread in the United States. By 1950 a substantial part of the country was covered by contracts with commercial cloud seeding groups, most frequently to increase rainfall. My own first contact with cloud seeding goes back to that year, when the Division of Water Resources, Department of Public Works of the State of California, requested the Berkeley Statistical Laboratory to evaluate the claims of success of commercial cloud seeders (see ref. 5).

The following occurrences mark some of the important developments of the weather modification problem in the United States. In 1957, a two-volume report of President Eisenhower's Advisory Committee on Weather Control was published (6). In 1963, the National Academy of Sciences (NAS) formed a Panel on Weather and Climate Modification and in 1964, the National Science Foundation (NSF) established a Commission on Weather Modification. In the same year, the NAS Panel published its first report (7). Two years later, this publication was withdrawn and superseded by another report (8). At about the same time, weather modification hearings were held before the Committee on Commerce of the U.S. Senate. The proceedings of these hearings appeared in 1966 (9). In 1969 the situation was reviewed from a university community's point of view (10). In 1973, there appeared another publication of the NAS National Research Council (NRC) Panel (11).

On a different level, a very important development in the field of weather modification occurred, apparently in 1962 (12), as a result of an action by the 87th Congress. This was the establishment of the Bureau of Reclamation's Atmospheric Water Resources Research Program, generally known as the Skywater Project, for a long time under the leadership of A. M. Kahan. According to Kahan (12), the broad objective of this program was to ascertain whether it is economically feasible to increase the water supply available to reclamation projects. In addition to funding a large number of cloud seeding programs, frequently in cooperation with the NSF and other governmental units, the activities of the Bureau of Reclamation included a series of conferences labeled "Skywater Conferences." The proceedings of these conferences are very informative.

Still another government-supported organization must be mentioned. This is the National Center for Atmospheric Research (NCAR) located at Boulder, CO. One of the interesting activities of NCAR was the organization and performance of the National Hail Research Experiment (NHRE).

As indicated at the outset, the reliability of the emerging cloud seeding technology is a very controversial matter. Here, a most laudable effort to achieve some sort of consensus deserves attention. In 1975, the editors of the *Journal of Applied Meteorology*, presided over by R. Robert Rapp, published a special issue (13) motivated by the

... great diversity of opinion as to the degree to which man can use the available tools of science to induce changes in his meteorological environment. On the one hand, there are those who argue that techniques of modifying weather have been demonstrated successfully for so many years that the time has now come for a major push into the application stage. On the other hand, there are some who hold the viewpoint that the feasibility of significantly altering certain important meteorological parameters (such as precipitation) has not been *scientifically* determined, and that intensive well-designed experimentation is still very much needed to evaluate properly the potential usefulness of modification technology (p. 652).

Since the early 1950s there has occurred a considerable change in the public acceptance of cloud seeding as a means of alleviating precipitation shortages. Originally, operational (that is, not experimental) projects were paid for by the customers. There were many such projects. Contrary to this, in recent years, a substantial part of the cost of operational cloud seeding has been borne, or is expected to be borne, by the federal and/or local governments. For example, on p. 25 in the Appendix of ref. 14 is the following statement: "About half of the funding requirements is to be through the federal sources, primarily the Bureau of Reclamation, and about half is to be from state and local sources, including counties."

A recent publication of the Bureau of Reclamation (15) illustrates the fact that federal funds are now being used, also, for promotion of the cloud seeding industry. The promotion effort is deep and reaches local college students. The (questionable) title of a section on p. 7 of that publication reads: "Colorado River Basin research confirms mountain snowfall can be increased." Details will be discussed below.

On the other side, there now exist local groups of interested citizens who advocate legislation to prohibit cloud seeding. Recently, one such organization, complete with a President, a Vice-President, a Secretary, and a Treasurer, circulated a leaflet (16) under the telling title: "Cloud seeding, the crime of the century ... dishonesty in cloud seeding community." Obviously, the problem of weather modification requires a careful, objective reevaluation.

UNRELIABILITY OF OPERATIONAL CLOUD SEEDING IN PROVIDING EVIDENCE OF EFFECTIVENESS

Because of the notorious variability of weather manifestations, it is now generally recognized, at least on the level of lip service, that the only reliable information on the effectiveness of cloud seeding, by a specified method in specified conditions, is an experiment with strict randomization. Because commercial cloud seeding is "operational" (that is, nonrandomized), our response to the request of the California Division of Water Resources of 1950 consisted of the identification of the mechanism through which the contemporary method of evaluation could lead to wrong conclusions. Briefly, this method of evaluation, labeled "historical regression line," is this.

To estimate the effect of seeding it is necessary to estimate the long-range average of seeded precipitation in the contracted target, a sample of which is directly available, and also the similar long-range average of what would have fallen without seeding, for which no direct data exist. Thus, this latter estimation must be indirect and involves two arbitrary choices. One is the choice of the so-called "historical period" when there was

no seeding. The other is the arbitrary choice of the so-called "control" area. The reader will notice that the precise meaning of "target" and "control area" is two sets of rain gauges functioning in the two localities. When these two choices are made, the observed historical precipitation amounts, averaged over gauges in the target and in the control areas, are used to estimate the linear regression equation of the historical target precipitation on that in the control. Next, this historical regression line and the operational period's precipitation in the control are used to estimate the mean precipitation in target to be expected without seeding.

Thus, the question of reliability of the historical regression method reduces to the question of whether the target-control regression line computed for the historical period is necessarily valid for the period of contract for operational seeding. With the effective help of E. M. Vernon, then the chief weather forecaster in San Francisco, it was established in the early 1950s that the target-control regression line depends upon types of storms and that the frequency of different types of storms can vary from year to year. In some cases, the identification of different types of storms is easy. One example is the Carizzo Plain cloud seeding project. Here, the target and control areas were on the two sides of an approximately north-south directed chain of mountains. With more complicated topography, the "typing" of storms proved difficult. These findings of Vernon (17) were confirmed by Thom (ref. 6, p. 34), who claimed a more effective typing of storms for the Carizzo Plain project than that of Vernon. Furthermore, Thom deserves credit for discovering that, in addition to the phenomenon of "types" of storms, the validity of the historical regression line method can be undermined by the arbitrariness in the selection of control gauges. "If one takes the liberty of choosing among minimum distance controls, he can *often* [emphasis added] find any result for seeding that suits his purpose, either positive or negative" (p. 33, ref. 6).

Curiously, the two findings just described did not prevent Thom from performing historical regression evaluations of a large number of commercial cloud seeding projects, almost invariably "positive," that formed the basis of the distinctly optimistic report (6).

The priority in documenting, in an open journal, the non-reliability of the historical regression method seems to belong to Brier and Enger (18). In their paper in 1952, they presented two evaluations by this method of the same seeding operation. Two different historical periods were used, each with its own set of control gauges and different from the other. The results of the two evaluations were contradictory. However, the published findings did not prevent Brier from using the historical regression method in studies of nonrandomized commercial operations indicating positive effects of cloud seeding.

UNRELIABILITY OF REPORTS FROM SEEMINGLY AUTHORITATIVE SOURCES

As documented in ref. 5, many seemingly factual publications on rain stimulation are open to the criticisms of incompleteness and selectivity. As a result, these reports do not document much on the phenomenon of cloud seeding effects. What is documented is that, out of the factual material accumulated through an experiment, it was possible to *select* subsets consistent with certain assertions. Two illustrations must suffice.

(i) **The Whitetop Experiment.** The 5-year-long Whitetop experiment was organized and performed by R. R. Braham, Jr. Of the following five quotations arranged in chronological order, two (*b* and *d*) stem from the NAS. They appear to be

contradicted by two statements (*a* and *c*) by the experimenter, Braham, and by a more recent statement (*e*) of two meteorologists, Simpson and Dennis, involved in cloud seeding.

(*a*) *Original statement of Prof. Braham.* The first public statement of the findings of the Whitetop experiment seems to be Braham's paper presented at the meeting of the American Meteorological Society held in Reno, NV, in October 1965. On p. 2 of the handout (19) there is the following statement:

Immediately obvious is the fact that during most years the seeded plume [the plume of silver iodide smoke] experienced less rain and less echo than the corresponding non-[seeded] plume. But it is also evident that the non-plume on seeded days also averaged less rain and echo than on non-seeded days.

This statement of facts is followed, in the handout, by a discussion of the possible underlying mechanism, culminating, on p. 7, with a section "Conclusions and Speculations."

(*b*) *Statement on the part of the NAS.* On Feb. 29, 1966, G. J. F. MacDonald testified before the U.S. Senate Committee in his capacity as Chairman, Panel on Weather and Climate Modification, NAS (see ref. 9, pp. 144–155). A paragraph (p. 147) read as follows:

I next turn to the seeding of cumulus clouds. The University of Chicago Whitetop project has studied the precipitation from shower-producing summertime cumulus in southeast Missouri over a period of 5 years. Preliminary analysis of the experiments which were properly randomized in the statistical sense showed an increase of 5 to 10 percent of the precipitation in the seeded clouds.

(*c*) *Braham's testimony.* A few days later, on Mar. 8, 1966, Prof. Braham also testified before the same Senate Committee (ref. 9, pp. 289–301). A part of his exchange with Senator Dominick (pp. 295–297) is very relevant to the formulation of national policy on weather modification. The part that refers to the Whitetop experiment (p. 297) reads:

"Yet, as a consequence of that long series of studies, we now come to realize that indeed there may be periods in the weather, certain weather situations in southern Missouri, in which *our seeding*, using standard seeding techniques, *resulted in decreases* in the precipitation in that region . . . Now it is perhaps only an accident that you [Senator Dominick] have chosen Oklahoma . . . because it so happens that in the weather conditions which in Missouri appeared to be favorable for these *decreases*; in fact, they are more common in Oklahoma than they are in Missouri." [Emphasis added.]

(*d*) *NAS-NRC Report of 1966.* The following quotations about the Whitetop experiment are from p. 19 of ref. 8.

Although initial inspection of the data suggested no marked effects of seeding, more recent analyses of radar precipitation echoes (Braham, 1965) [ref. 19 in this paper; see quote *a* above] reveal some *evidence for positive seeding effects* (order of 5 to 10 percent increase of radar echo frequency) in the region lying just downwind of the seeding arc, changing over to negative effects of about the same order of magnitude . . . and returning to positive . . . However, on the basis of the *preliminary results*, it would appear that *modest precipitation increases* in summer cumuli in at least one portion of the continental interior of the United States *can be stimulated by silver iodide seeding*. [Emphasis added.]

The paragraphs that follow are relevant to the problem of national policy on weather modification.

(*e*) *More recent opinions of the cloud seeding community on the results of the Whitetop trial.* This may be exemplified by the following brief statement from ref. 20, p. 66:

Nevertheless, the deduced *seeding effect was negative, beyond reasonable doubt*, and the possible explanatory hypotheses may ever evade firm proof. [Emphasis added.]

The Berkeley Statistical Laboratory expended quite an effort on the study of the Whitetop experiment (21–24). In a sense, our final conclusions relevant to the appraisal of weather modification technology were less pessimistic than the con-

clusion of Simpson and Dennis (20) just quoted—namely, *not proven*. The present writer is convinced that Prof. Braham did his best to randomize his experiment properly and for quite some time we took the randomization for granted. On this basis we found what appeared to be evidence of large and unbelievably widespread losses of rain ascribable to seeding. At the end, however, while verifying a hypothetical mechanism of those losses (24), we found convincing evidence of a fault in implementing randomization that could have produced the observed seed-day deficiencies of rainfall in the target. In consequence, the scientific results of the Whitetop trial appear to be an illustration of the difficulty of implementing randomization. As interested reader may wish to examine pp. 12–14 of Braham's report (25), which may inspire explanatory speculations.

[CR] Comments of several scholars, including Prof. Braham, dispute the conclusion that our findings constitute "convincing evidence of a fault in implementing randomization." My effort to summarize the arguments within reasonable space proved unsuccessful. In consequence, the present insertion is limited to recording the existence of complicated disagreements which include questions of statistical methodology. The reevaluation of the Whitetop experiment is likely to be beneficial to the development of a reliable cloud seeding technology. [CR]

(*ii*) *Discrepancies Regarding Evaluations of Commercial Cloud Seeding Operations.*

(*a*) *Testimony of Gordon J. F. MacDonald.* While testifying (Feb. 29, 1966) before the U.S. Senate Committee in his capacity as Chairman of the NAS Panel on Weather Modification, MacDonald declared (ref. 9, p. 147):

"During the course of its investigation, the National Academy of Sciences Panel *obtained over a hundred reports* of commercial rain-making operations. The Panel then selected certain of these reports *for an independent evaluation*. Fourteen of the selected reports were concerned with projects in the Eastern and Middle Atlantic States and covered a wide range of meteorological conditions. The projects ran from 19 days to 5 months and in all but one of the 14 cases there was an increase in precipitation, with an average increase of about 15 to 20 percent. *This result was so striking* that the Panel asked for and obtained additional studies by the Rand Corp. and the U.S. Weather Bureau. *These independent groups substantiated the Panel's results.*" [Emphasis added.]

As will be documented below, the essential parts of this statement are inconsistent with information in the NAS-NRC report of 1973 (11) and with statements of individuals who participated in the reevaluation. Specifically, the commercial cloud seeding operations reevaluated by the Panel (two sets of evaluations, one of 14 items and the other of 11 items) do not represent the result of some kind of general survey performed by the Panel but were chosen out of those offered by a single commercial cloud seeding operator, Wallace E. Howell. Furthermore, the reevaluations published by the Panel (at least the second set) were not "independent" but were performed with Howell's active participation and advice. Finally, the asserted substantiation of the Panel's results by the Rand Corporation, etc., is subject to interpretation. Here, the interested reader is advised to examine the relevant appendices in ref. 8.

The two sets of evaluations differed in purpose. The first set refers to the effects of seeding on precipitation in targets under contract with customers. The second set was performed to substantiate the claim that (possibly some of the same) cloud seeding operations increased the rainfall in areas not covered by contracts, some of them up to 240 kilometers away. Here, then, there was another opportunity for arbitrary choices: not only of the control areas and of historical periods but also of the far away uncontracted "targets."

(b) *Passages from NAS publication of 1973 (11), pp. 123-124, inconsistent with the statement by MacDonald.*

The projects whose seeding effects were examined by Brier *et al* (1967) were non-randomized, whence the analysis technique selected was a regression method. The shortcomings of evaluations of this type are well known, but the findings ask for attention for several reasons. (1) The selection of Weather Bureau rain gauge records for use in these evaluations was made independently by the investigators, *not by the commercial seeder*. (2) The total number of seeding projects was 11, the total number of project months comprised the equivalent of almost 5 years of seeding, and the targets ranged in locale from South Carolina to New Hampshire. (3) The projects were spread in time over slightly more than a decade. [Emphasis added.]

It is seen that, according to this statement, the projects were not chosen by the NAS Panel, out of the "more than one hundred" assertedly available. The choices by the Panel appear to have been limited to "rain gauge records" and, possibly, to the nonidentified "commercial seeder."

(c) *Role of Wallace E. Howell as the commercial cloud seeder whose projects were evaluated by the NAS Panel.* At the International Symposium on Uncertainties in Hydrologic and Water Resource Systems held in Tucson, AZ, in November 1972, the discussion of the review paper (26) authored by E. L. Scott and the present writer brought about a quotation from a letter by D. B. Kline to Glenn W. Brier who, in cooperation with T. Carpenter, performed the evaluations published by the Panel. The particularly relevant part of this letter (ref. 26, p. 1242) is reproduced here.

Since I was working closely with Don Gilman (somewhat privately in order to help him and the, then, Weather Bureau) who in turn was participating (with Bob White) in the NAS Panel deliberations, I was perhaps more aware than you of the background that led to our use of the projects treated in our joint paper with Tom Carpenter *et al*. Simply and directly stated, those were the only project reports readily available to us that were based on a single operator's activities (and therefore presumably a reasonably cohesive set of data) that were at the same time in the public domain. You may recall that Wally Howell had over the years sent copies of nearly all of his project reports to the Weather Bureau Library. *The few that were missing Wally graciously supplied to us.* [Emphasis added.]

Here, it is relevant that, while appearing before the U.S. Senate Committee (ref 9, p. 248), Howell asserted that "In the past 16 years I have been involved in some 60-odd weather modification programs . . ."

(d) *Wallace E. Howell's own statement.* On the first page of his paper (27), Howell stated:

Operations intended for local effect were also examined for signs of effects at a distance. *With the author's cooperation*, Brier, Carpenter, and Kline (1967) [ref. 28] analyzed precipitation downwind from a number of commercial precipitation stimulation operations and found evidence of increases extending over 250 km. [Emphasis added.]

Using the historical regression method, combined with certain statistical innovations too long to describe here, the Brier *et al.* paper (28) does support the claims of success of commercial cloud seeders. It will be remembered that Brier has the priority of documenting the nonreliability of the historical regression method.

[CR] A letter from the Editor of PNAS informs me that, according to G. J. F. MacDonald, the specific commercial projects evaluated by the NAS Panel were selected by J. E. McDonald. Here it is appropriate for me to admit hastiness in the assertion in the original draft of this paper that the selection of projects evaluated by the NAS Panel was done by Howell. All that appears to be documented by the above quotes from the Panel's publication (11) is that the investigators of the Panel, not the cloud seeder, selected the rain gauge records. The question of who performed the selection of projects remained unresolved.

Regretfully, the subsequent efforts at documentation proved unsuccessful.

J. E. McDonald died in 1971. In the preface to ref. 8, Chairman G. J. F. MacDonald expressed appreciation of J. E. McDonald's prolonged efforts to "a study of reports of operational cloud seeders." This may or may not have included the selection. The articles that G. J. F. MacDonald and J. E. McDonald contributed to Fleagle's volume (10) do not contain information on this particular point. However, these two articles may well be relevant to the formulation of the national policy. These articles are very different.

The paper by G. J. F. MacDonald (pp. 69-86) appears to be mainly concerned with legislation. The proposal is to create a new governmental agency empowered to coordinate operational weather modification programs in other agencies, etc.

The paper by J. E. McDonald (pp. 43-55) bears the title "Evaluation of weather modification field tests," but the contents are different. Apparently, there were some differences within the NAS Panel and J. E. McDonald was in the minority. The following disconnected quotations illustrate his emotions: (a) ". . . enthusiasm should be based on facts . . .," (b) "Type 2 antistatisticians . . . are capable of influencing decisions in high places . . .," (c) "If we do not avoid any and all misrepresentations . . ." [CR]

RELIABLE INFORMATION ON EFFECTS OF CLOUD SEEDING MAY RESULT FROM STRICTLY RANDOMIZED EXPERIMENTS

With reference to precipitation augmentation, the essence of a randomized experiment is, briefly, as follows.

First, "potential experimental period" (or "seeding opportunities") and the "response variable" are clearly defined. In the simplest case, the potential experimental period may be of fixed duration, say 24 hr from 0730 of a given day to 0730 of the next. In this case, the response variable might be the precipitation measured by specified gauges (defining the "target"), say from 0800 of the given day to 0800 of the next.

A special organizational unit, to be called the "randomization center" (RC), must be established. At an appointed time before the beginning of a potential experimental period, the experimenter reports to the RC whether the approaching potential experimental period is suitable for inclusion in the experiment—that is, whether it is to become an "experimental unit." In the affirmative case, the experimenter communicates to the RC certain other information deemed important, such as the nature of the prevailing weather (type A, type B, or type C, etc.). In response, the RC provides the experimenter with a randomized decision, either a permissive "seed" or a categorical "do not seed." It is emphasized that the randomized decision must be communicated to the experimenter AFTER his declaration as to the approaching experimental unit, not before. In fact, it would be best to arrange that even the personnel of the RC have no advance information on the nature of the forthcoming randomized decision. Perhaps, a computerized random number generator could be adjusted for this purpose.

All of these exchanges between the experimenter and the RC must be automatically recorded, perhaps by using teletype machines. The records at both ends must be consistent. The primary evaluation of the experiment must be based on *all the experimental units* (some seeded and others not) and no others, and it must use the originally defined response variable. The supplemental information about the type of weather ought to be used for stratification purposes and is useful by providing the experimenter with means to verify his ideas.

If the above principles are strictly observed, then the competently developed statistical tests will guarantee a preassigned frequency of errors ("level of significance" α) in asserting that the observed difference between seeded and not-seeded precipitation amounts is not due to vagaries of randomization. Here, as everywhere in research, a degree of skepticism is a healthy attitude. Fortunately, the widely accessible digital computers make it relatively easy to use the Monte Carlo simulation techniques to study the statistical tests.

In addition to satisfying the maintenance of the chosen level of significance— $\alpha = 0.10$ or 0.05 or 0.01 , etc.—it is important to investigate the so-called "power" of the test. This would answer questions of the following kind: With the given level of significance α and the given number of experimental units, say $n = 100$ or $n = 200$, etc., what is the chance, β , of detecting a real seeded-not-seeded difference in precipitation if it represents a 20% (or some such) decrease (or increase) in the rain due to seeding? If the calculated $\beta = 0.1$ or 0.2 and the 20% effect is all that is anticipated and deemed really important, then the contemplated experiment can hardly be considered promising and changes in its design would be in order.

Remark. In the past, the requirement emphasized above—that the experimenter should make his decision about the approaching potential experimental period without advance knowledge of whether it would be seeded or not—has been frequently ignored. It is, then, a pleasure to see that the importance of this requirement begins to be appreciated by the cloud seeding community. The following quotation is from p. 13 of chapter 6, ref. 29: "Even the most sincere investigator may subconsciously mis-identify a possible seeding opportunity if he knows whether the opportunity, if declared, will be actually seeded or merely kept as control."

The difficulty visualized here is that of subconscious self-deception of the experimenter, unavoidably followed by deception of the public. Difficulties of this kind are particularly great with experimental units represented by single clouds, some intended to be chosen at random to be seeded and some others to serve as controls. An apparent case of self-deception has been noted by Langmuir (ref. 30, p. 552). I am indebted to Alexander Brownlee for calling my attention to this article.

[CR] One of the most informative American cloud seeding experiments was performed by L. J. Battan (31). However, a partial neglect of the principle, "declaration of seeding opportunity first, randomized decision later," creates an uncertainty regarding about one-half of Battan's results. Hoping for an increased precision in evaluations, Battan decided to randomize his experiment in pairs of "suitable" days. The randomized decision as to whether the first day of a pair should be seeded or not was reached *after* the suitability of this day was determined. However, the decision for the first day implied the contrary decision for the second day of the same pair. The two days of a pair were allowed to be separated by no more than one "unsuitable" day. If two consecutive days, after the first day of a pair, were found "unsuitable," then the originally declared first day of a pair was eliminated from the experiment.

Here, then, the decision as to whether a potential "second" day of a pair is "suitable" or not was reached with full knowledge of whether it would be seeded. Because of this first day-second day difference, our studies of the experiment (32–34) had three parts: all days, first days, and second days. It happened that the general pattern of the results for second days was different from that for first days. The unresolved question is whether this difference reflects an interesting atmospheric phenomenon or a subconscious difference in Battan's judgments about the suitability of days, or both. Certain more recent

efforts to use the discarded "first days" that frequently followed "second days" of earlier pairs, particularly to study the relationship between the rain of the "discarded first day" and that of the preceding "second day" are similarly doubtful. [CR]

PHENOMENON OF FAR-AWAY EFFECTS OF LOCAL CLOUD SEEDING

As described in some detail in ref. 5, the studies of the late 1960s indicated the following unexpected fact. Cloud seeding conducted over moderate-sized targets, 20–50 kilometers across, can have strong apparent effects on precipitation reaching the ground in areas at distances on the order of a couple of hundred kilometers from the boundary of the intended target. First, this phenomenon was noticed (35) in the randomized Swiss hail prevention experiment, Grossversuch III (36). The seeding of thunderstorm clouds was performed in the canton Ticino on the southern slopes of the Alps. This was done during seven consecutive summers, 1957–1963, by using AgI smoke generators mounted on tops of mountains surrounding the target. The discovery occurred during our efforts (37) to verify certain basic hypotheses on the mechanism of cloud seeding effects. In order to increase the precision of evaluations, some good control areas were needed. Seven such areas were tried. Two were in Switzerland, each with 20 rain gauges judged reliable, and five were in Italy, with the number of gauges varying from 7 to 15. The usefulness of a prospective control area depends on the absence of any sign of effects of seeding.

It happened that substantial and occasionally significant apparent effects of seeding in Ticino were found for practically all of the areas tried. The sign of these apparent effects happened to depend upon the presence or absence of "warm" stability layers: in the presence of such layers, there were apparent increases and in their absence (i.e., with "uninhibited updrafts") there were apparent decreases in rainfall ascribable to seeding. The apparent effects depended also on wind directions. For example, with southerly winds at 1500 m above sea level, the seeding in Ticino on days with stability layers appears to have more than doubled the precipitation near Zurich some 130 kilometers to the north (two-tail $P = 0.004$), but not with northerly winds, etc.

While the above findings appeared convincing, their practical significance depends on the generality of the phenomenon. In other words, the question arose as to whether anything of this kind occurred in any other cloud seeding experiment. Also, what might be the mechanism of the phenomenon? Finally, what might be an efficient method of study to verify the presence or absence of the phenomenon?

Just these questions motivated our persistent effort to study the Whitetop experiment, as mentioned earlier. When the discouraging fault in implementing randomization was discovered (19), the other promising experiment appeared to be that in Arizona (1957–1964) performed by L. J. Battan. As a result of three studies (32–34) it was found that, indeed, Battan's seeding of clouds over the Santa Catalina Mountains was accompanied by significant and occasionally "highly significant" apparent effects on rainfall in downwind areas up to the distance of some 270 kilometers. However, there was a difference from what was found for the Swiss experiment. In Switzerland the sign of the apparent downwind effect depended upon the presence of warm stability layers. On the other hand, in Arizona, all the apparent *downwind* effects were negative: large decreases in rainfall.

However, large and significant apparent increases in rainfall ascribable to seeding were found in equally far-away areas that were "on the right" of the day's wind direction. As described

in some detail in table 1a of ref. 26 a spot check of these general results showed, among other things, that on the 39 experimental days when a group of gauges in New Mexico was downwind from Santa Catalinas, the average seed-day precipitation was less than on control days by 63% of the latter.

The absence of positive downwind apparent effects of seeding, in Arizona, is likely to be due to the method of seeding. As mentioned, in Switzerland the seeding was done from the ground so that, in the presence of warm stability layers, the seeding material was temporarily kept under a lid. On the other hand, in Arizona, the AgI smoke was dispersed from a plane, presumably at levels above the warm stability layers, if any existed. Thus, the seeding in Arizona must be comparable to seeding in Switzerland on days with uninhibited updrafts.

The findings of the far-away effects of local cloud seeding are discussed in the 1973 NAS Panel Report (ref. 11, pp. 125-129), ending with the recommendation of a searching inquiry. "The scientific payoff that could conceivably lie in these puzzling indications could be quite great."

The importance of the phenomenon, both from the point of view of national weather modification policy and from that of current efforts to alleviate drought, can hardly be over-emphasized. Regrettably, thus far, no effort seems to have been made to verify our findings.

A brief description of the hypothetical mechanism of the far-away apparent losses of rain is given in ref. 5. A more detailed publication on this subject is contemplated.

[CR] Certain comments reflect interest in the timing of the far-away apparent effects of local cloud seeding, particularly with reference to the direction of winds aloft. These questions were extensively studied in Berkeley. Fig. 1 exemplifies the findings for Zürich, about 130 kilometers away from the target of Grossversuch III (38), and for Walnut Gulch, about 100 kilometers from Battan's target in Arizona (32). [CR]

RECENT INFORMATION ON CLOUD SEEDING TECHNOLOGY RELEVANT TO ALLEVIATION OF DROUGHT

All of the preceding discussion is based on experimental data obtained in the late 1950s and early 1960s, essentially on Grossversuch III, Whitetop, and the Arizona experiment. The purpose of the present section is to examine the more recent thinking and the more recent experiments. The time covered extends from 1972 (Skywater Conference VII) to 1977.

Skywater Conference VII. The Proceedings (14) of this conference is important because it illustrates the contemporary thinking of the Bureau of Reclamation (personalized by A. M. Kahan) and of a number of invited participants. The Conference was held to discuss the proposed gigantic "regional" operational cloud seeding project extending over Nebraska, South Dakota, North Dakota, Montana, and Wyoming. The purpose of the project was to increase the rainfall from the summer cumulus clouds and to decrease hail. Two questions discussed are relevant to the present paper. One is whether randomized experiments are still needed. The other is whether the existing evidence justifies the hope that the proposed seeding of summer cumulus clouds will increase the precipitation.

Mr. Swenson: How about research in a larger area? . . .

Chairman Kahan: We need here to recognize that nobody is advocating a cessation of research . . .

Dr. Weinstein: What about reduction?

Chairman Kahan: This might be a consequence if you have limited resources available. The expansion in one area could imply reduction in another.

Dr. Woodley: Would the operations include control areas and programs, some sort of a weighted randomization?

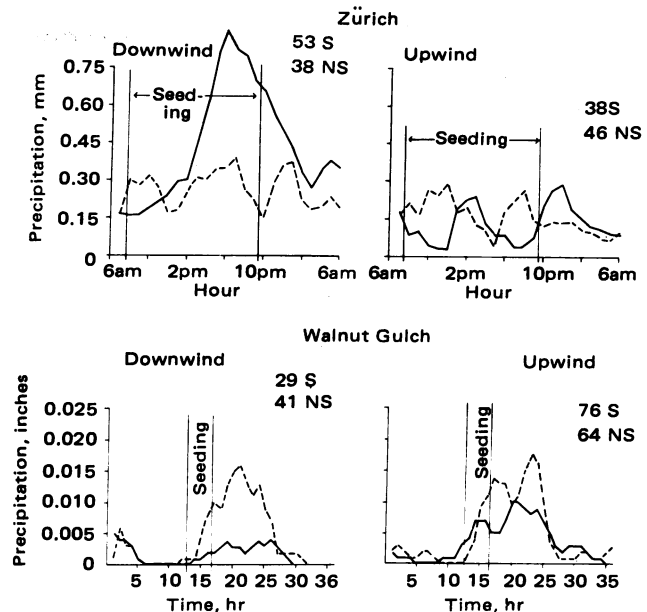


FIG. 1. Diurnal variation in hourly rainfall with seeding and without, observed in Zürich during Grossversuch III (with stability layers) and in Walnut Gulch during the Arizona experiment, when these localities were (approximately) downwind and when they were upwind. Curves show 3-hr moving averages; —, seeded (S); - - -, not seeded (NS).

Chairman Kahan: It will include evaluation efforts. *I don't think that you are going to be treated to the luxury indefinitely of people accepting randomization.* . . . [p. 84; emphasis added.]

As to the prospect of increasing precipitation by seeding summer cumulus clouds, the most outspoken pronouncements came from L. O. Grant, the leader in the performance of the two Climax experiments.

Mr. Grant: . . . on the next scale up which is an area-type experiment which is smaller than regional, there have been experiments carried out, the Arizona, Whitetop, and some of the South Dakota experiments in this category. There have been a number of experiments carried out, and as far as I can tell **THERE HAS NOT BEEN ONE OF THESE CARRIED OUT THAT HAS GIVEN A CLEAR PICTURE ON POSITIVE RESULTS.** [Forceful emphasis added!]

The model said it should be there. But there really hasn't been so far as I can tell a single one of these carried out with the positive result . . . But we have run a summer-cumulus-type experiment also. Our results are not that much different than Arizona, Whitetop, or most of the South Dakota experiments. So I think we need to clean up our models and run a good area-type experiment . . . [pp. 79-80; emphasis added.]

Much of the current cloud seeding literature relies on mathematical models of precipitation and of effects of seeding. They are programmed for digital computers and are variously used for evaluation. The models seen by the present writer appear simplistic. The emphasized sentence of Grant expresses his own skepticism.

Pyramid Lake Experiment. The 3-year-long Pyramid Lake experiment, concluded in 1975, was organized and conducted by P. Squires of the Desert Research Institute (DRI), University of Nevada, Reno, NV. The Berkeley Statistical Laboratory (BSL) served as the randomization center and performed the preliminary evaluation.

The purpose of the experiment was to determine whether the seeding of the winter orographic clouds passing over the crest of the Sierra Nevada could increase the precipitation in the catchment of the Truckee River which flows into the Pyramid Lake. The intended target was an area around Lake Tahoe. The actual target was represented by a set of sensors.

The rugged mountainous terrain forced the distribution of the sensors to be far from uniform.

[CR] I am indebted to K. A. Brownlee for the constructive remark that, in order to have a fully realistic measure of precipitation in the intended targets, the distribution of the available sensors should be randomized. [CR]

The experimental unit was a "storm." The messages of DRI to BSL included the "predicted duration" of the approaching storm and also an appraisal of its intensity: A = strong, B = medium, C = weak, and D = marginal. (However, there were only nine D storms and they are ignored here.) The thus far preliminary evaluation showed that the seeding of the combined A + B category resulted in an apparent 25% increase in precipitation ($P_1 = 0.31$). Contrary to this, the apparent effect of seeding C storms was a 48% loss of precipitation ($P_2 = 0.30$). Although both results are far from being significant, it is encouraging that the assertion of no real effects, for A + B and also for C storms, has the significance probability $P_3 = P_1 P_2 = 0.09$. The contemplated further study of the experiment includes the use of certain predictor variables. However, to be really informative, the experiment should be continued for a few more years.

Incidentally, the analysis of data revealed a problem of the sensor manufacturing industry: sensors of precipitation are needed that function reliably during severe winter storms.

Colorado River Basin Pilot Project. Information on this project, frequently called San Juan Mountains Project, stems from the 641-page report (29) dated October 1976. The project was organized by the Bureau of Reclamation. The experimental seeding continued over five winter seasons (1970/71–1974/75) and thus has some analogy with the Pyramid Lake.

The complexity of the San Juan project, involving several private companies and a consultant, A. Court, appears impressive. Pages 1–12, concerned with methods, mention methodological instructions of the Bureau of Reclamation that varied "from year to year, and even during years." The summary of the results is stated twice. On p. 1-1 there is the following statement:

During this project 320 kilograms of silver iodide were released on 71 days. These days were chosen at random from the 147 days declared to be suitable for efforts "to produce positive increases in snowfall over large areas of the San Juan Mountains. . . ." Such increases apparently were produced on some days, but were countered by equivalent decreases on other days.

The following passage is quoted from p. 6-1:

The principal conclusions arrived at on the basis of the overall evaluations are:

1) There was no significant difference in the net target area precipitation on seeded experimental days and that on not-seeded experimental days. This null effect was *apparently* due to the mixing of positive and negative seeding effects." [Emphasis added.]

These two statements of fact do not appear quite consistent with the assertion, "Colorado River Basin Research *Confirms* Mountain Snowfall Can be Increased," quoted earlier from the recent publication of the Bureau of Reclamation (15). [Emphasis added.]

The body of the report gives precipitation data for a large number of sensors, including those that remained in their locations over the whole period of the experiment and also others that were moved or operated only part of the time. Upon receiving the report, the Statistical Laboratory performed three evaluations, all based on data from sensors that operated over all the five seasons and were not moved. One evaluation was for the precipitation in the target, one separate evaluation covered all the gauges outside the target, and the third used all the data combined. The results are shown in Table 1.

Table 1. Evaluation of Colorado River Basin Project

| Area | % effect | Two-tail P |
|--------------------------------|----------|------------|
| Target only, 18 sensors | –12 | 0.536 |
| Out of target, 31 sensors | –4 | 0.820 |
| Target and outside, 49 sensors | –8 | 0.668 |

Although the somewhat chaotic performance of the experiment is clearly regrettable, the authors of the report deserve compliments for the realism of presentation and for providing day-by-day precipitation data. The repeated changes in the definition of "suitable" day create difficulties in reliable stratifications.

National Hail Research Experiment. The report (39) was written by D. Atlas, who has been the director of the National Hail Research Experiment (NHRE) mentioned earlier. Although primarily concerned with hail, the 3-year-long experiment also collected data on rain. The results of the preliminary analysis (ref. 40, p. A-6) indicate that on seeded days there was more hail and more rain than on days without seeding.

In his article, Atlas presented a substantial review of the relevant literature, pointing out contradictions between many enthusiastic reports of success in suppressing hail and quite a few indications of failure. This was followed by a discussion of underlying processes in the atmosphere. For purposes of the present paper, the following passage is relevant.

. . . some of these implications have been *obscured in part* by the early encouraging results in some parts of the world, *by implicit pressures for positive results*, and by the difficulty of recognizing and admitting negative results in some programs when others are claiming such drastic positive ones.

Indeed, *this dilemma has characterized many efforts to modify weather*. When positive results are obtained, even at inadequate levels of statistical significance, the unconscious bias of the investigator or the operator inclines him to put them in the best possible light. On the other hand, when negative results are found, there are two common reactions: (i) to search far and wide for physical or statistical reasons to account for them, a process which frequently succeeds in a plausible if not entirely true explanation; or (ii) to attribute them to inadequate or inappropriate methods. [p. 143; emphasis added.]

If there were any "implicit pressures for positive results" in NHRE, Atlas did not yield. He resigned.

Florida Area Cumulus Experiment. The report (41) was written by Woodley and Berkeley of the National Oceanic and Atmospheric Administration (NOAA) and Simpson and Biondini of the Department of Environmental Sciences at the University of Virginia. It summarized the rainfall results obtained by the Florida Area Cumulus Experiment (FACE) during the summers of 1970–1975. The following two quotations must suffice.

. . . these experiments demonstrated beyond reasonable doubt that "dynamic seeding" is effective in increasing the sizes and lifetimes of individual cumuli and the rainfall from them . . . [p. 735.]

Analysis . . . of the 48 random experimentation days obtained through 1975 provided no evidence that dynamic seeding appreciably altered the rainfall over the fixed target area . . . [p. 742.]

The FACE experiment was concerned with a cloud physics problem of developing a new cloud seeding technique, dynamic seeding, but provides no evidence of success of this technique in the efforts to alleviate drought.

CONCLUDING REMARKS

The conditions of the present drought emphasize the urgency of a thorough review of present cloud seeding technology, as illustrated by the *Newsletter* of the Bureau of Reclamation (15).

The first page of the *Newsletter* shows a map of California

with 15 shaded areas marking operational cloud seeding projects, 7 of them identified by the State as "emergency" efforts. The same page informs the reader of the results of an opinion survey (of special groups), with 50% of the respondents answering "Yes" to the question, "Do you think cloud seeding works to actually increase snowfall?" Page 7 displays the title of a brief article, "Colorado Basin Research Confirms Mountain Snowfall CAN BE INCREASED." Earlier in the present paper, it was noted that this title is inconsistent with the actual findings. Page 8 shows a picture of a college girl holding a rawinsonde balloon, with the adjoining text recording this girl's impressions of certain briefings. The entire brochure marks an energetic and effective activity of the Bureau of Reclamation. But is this activity consistent with the objectives of the Bureau's Project Skywater, as described by Kahan (12)? See page 4.

Even though large expenditures on operational cloud seeding in California during the current year appear to have been already decided on, there is the question of subsequent years and of other localities. The problem of a careful evaluation of the present state-of-the art of cloud seeding appears to be urgent.

As to the subjects of study by the advocated (at least two) interdisciplinary research groups, a list of completed experiments, published in ref. 11 (pp. 227-258) might be considered. A more recent source of information is the Biennial Report of 1973-74 published by the Bureau of Reclamation (42).

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